

The Radiometer and its Lessons

PROF. OSBORNE REYNOLDS arranges his last letter (NATURE, vol. xvii. p. 220) under four numbered heads, and in the reply which I appear called on to make I will follow this division.

1. In the first section he says, "There is nothing in my earlier papers that is 'admittedly erroneous.' If there is error in these papers I am not aware of it." This is strange. In his first paper (*Proceedings, Royal Society*, vol. xxii. p. 40) Prof. Reynolds declares its object to be "to point out and to describe experiments to prove that these effects (the motions observed by Mr. Crookes) are the results of evaporation and condensation." Now they are not the results of evaporation and condensation: and it might have been seen, *ab initio*, that evaporation and condensation could have had nothing to do with them; for evaporation and condensation can only produce a temporary force, ceasing so soon as the distillation is complete, and cannot therefore be any part of the cause of a persistent force such as that detected by Mr. Crookes, which lasts for any length of time during which the heat is applied. In the same paper Prof. Reynolds further says, "The reason why Mr. Crookes did not obtain the same results within a less perfect vacuum [than that of the Sprengel pump], was because he had then too large a proportion of air, or non-condensing gas, mixed with the vapour, which was also not in a state of saturation." All this is manifest error. But this is not all, for the whole of the theory of those papers is erroneous; neither condensable vapour nor residual gas acts in the way described by Prof. Osborne Reynolds. In investigating the force arising from evaporation and condensation, he has overlooked the circumstance that the evaporation from the disc will keep back part of the vapour which would otherwise have reached it, and in investigating the effect of condensation he tacitly assumes that it does keep it back. Now in both cases the reverse of the assumption is what takes place, and he actually arrives at the absurd result that "if the opposite sides of a pith ball in vapour were in such different conditions [*i.e.*, one surface evaporating, the other condensing] the ball would be forced towards the colder side" (p. 404). His conclusion amounts to this: that the recoil of a cannon would be doubled if it were struck from behind by a missile at the same moment that it discharges an equal mass with equal velocity forwards! If he had not made these mistakes he would have got out only the forces which result from "the perceptible motion of the vapour," which he states "would be insensible" (p. 403), along with alterations in the general tension of the vapour which would act equally on both sides of the disc. Those errors vitiate the whole of his mathematical reasoning, so that the value for f which he gets, is not, as he supposes, "the force arising from evaporation," and his law connecting it with the heat falls to the ground. I have all along supposed, from Prof. Reynolds's having long ceased to mention his theory of evaporation and condensation, that he was aware of some of its errors.

The same error vitiates his reasoning in reference to the action of residual gas. If the error is corrected, and if, as he assumes, the gas coming up to the disc had been unpolarised (*i.e.* had brought to the disc equal numbers of molecules, and with equal velocities from all directions in front), his investigation would only have given him an increase in the general pressure of the gas, acting, as I pointed out in paragraph 5 of my first paper (*Phil. Mag.*, March, 1876, p. 179), equally on the front and back of the disc, except during the almost inappreciable instant of adjustment. Prof. Osborne Reynolds therefore wholly missed the source of the persistent force with which Mr. Crookes's experiments deal.

2. Prof. Osborne Reynolds next says that his second paper "does not conclude with his own expression of opinion that residual gas is not the cause of the force observed by Mr. Crookes." In reply I have only to quote the concluding words of that paper (*Phil. Mag.*, November, 1874, p. 391). After passing under review the two agencies (condensable vapour and residual gas), which he supposes are to be considered, he decides in favour of the former in the following words: "hence in such cases [*i.e.*, under the conditions which he supposed to prevail in Mr. Crookes's experiments] it seems to me that the effects must be due to the forces of condensation."

3. In the next section of his letter Prof. Reynolds states that Clausius and Maxwell "established the law that the only condition of thermal equilibrium in a gas is that of uniform temperature." I am not aware that they have ever established this law.

The converse of it is obviously true, and has often been used, and the law itself has sometimes been assumed, but has never, so far as I know, been proved. I am, however, disposed to concur with those who think that it is probably true, and the conclusion in my paper on penetration (which is the reverse of that attributed to me by Prof. Reynolds) is in conformity with it. My conclusion is expressed in the following words (*Phil. Mag.*, December, 1877, § 4):—"Hence there must, in the cases that really arise, be some escape of heat which may be small but cannot vanish." And, I may remark, there will, according to my view, be two other sources of escape of heat, *viz.*, conduction by diffusion, which was excluded from my investigation; and conduction by radiation, which was excluded both from Clausius's investigation (*Phil. Mag.*, June, 1862, p. 422, footnote) and from mine.

Prof. Osborne Reynolds a second time objects to my having excluded conduction when investigating the penetration of heat. As he attaches weight to authority he will perhaps be reconciled to my doing so, by the example of Clausius, as cited above, and by his justification of it in the following words (*loc. cit.*):—"In any case, however, it is allowable to consider separately each of these two ways in which heat moves."

Before passing from this subject I wish to take the opportunity of stating that Dr. Schuster's letter (NATURE, vol. xvii. p. 143) has satisfied me that I have hitherto erred in my estimate of the relative efficiency of penetration and conduction as agents for conveying heat. I am now convinced that penetration is usually feeble compared with conduction, and, in the figures representing De la Prevostaye and Desains' experiments, is to be sought in those portions of the curves which slope steeply downwards. The second part of my paper on penetration, that in which I apply the theory to experiment, will accordingly require considerable modification; and some of the statements which I made in my papers on Crookes's force will need amendment. The corrections that are required do not, however, affect any of the material parts of my theories of Crookes's force and of penetration, which depend essentially on the fact that there is a layer in the gas extending to a limited distance from a heater or cooler, throughout which the effects of the discontinuity in the gaseous motions at the surface will be felt, and that within that layer the stresses and the communication of heat follow special laws.

4. I have to express my great satisfaction at the explicit admission made by Prof. Osborne Reynolds in the fourth section of his letter, in the following sentences:—"There is one statement in Mr. Stoney's letter which is not erroneous. He says, 'I cannot find anywhere in Prof. Osborne Reynolds's writings an explanation of the thing to be explained, *viz.*, that the stress in a Crookes's layer is different in one direction from what it is at right angles to that direction.' I [Prof. Osborne Reynolds] do not at all admit that this is 'the thing to be explained,' and I am quite sure that Mr. Stoney would find no explanation of it in my writings." This admission disposes finally of all controversy as to priority between us.

I need hardly, after this admission, follow Prof. Osborne Reynolds through the rest of his letter. His supposed invariable law "that the [Crookes's] force always tends to drive the vanes or bodies in the direction of their colder faces," does not seem to be true. A familiar exception occurs when a spheroidal drop is supported over a platinum dish. The Crookes's force acting upon the platinum dish is equal to the weight of the drop, and acts downwards, *i.e.* in the direction of the *hottest* surface of the dish.

In applying his hypothetical case of a heater and cooler, A and B, within an envelope of intermediate temperature, to prove that "the force that causes the motion in the bodies cannot be due" to the stresses of my theory, he has overlooked the very obvious circumstance that the envelope, as well as B, is a cooler in reference to A, and the envelope, as well as A, is a heater in reference to B.

Prof. Reynolds observes that I have not defined polarisation. I described the kind of polarisation that exists in radiometers in my first two papers, and I will give a formal definition of the term as applied generally to gases in an article which I am preparing, and which I hope will be admitted into the pages of NATURE, giving as clear an account of my theory as I can, compatibly with brevity and the omission of mathematics.

The way in which Prof. Reynolds has excluded polarisation from his explanation is by assuming that the state of the gas close to the heated disc may be adequately represented by unpolarised gas of one temperature coming up to the disk, and unpolarised gas of another temperature leaving it, *i.e.*, by mole-

cules coming up to the heater in equal numbers and with equal velocities from *all* directions in front, and by molecules receding from the heater equally in all directions, although with augmented velocities. Under these circumstances there would be no difference in the pressure on the front and back of the disc, except during the very brief period of adjustment.

By making this assumption Prof. Reynolds leaves the part of Hamlet out of the play; for Crookes's force arises out of the very circumstance which has been omitted, *viz.*, that the molecules that come up to the heater or cooler, arrive in the form of a rain which predominates in a definite direction, a direction which is normal to the heater and cooler in the simple case of their being parallel.

G. JOHNSTONE STONEY

A Double Rainbow

ON the 28th inst., at about 6.30 P.M., while myself and some ten or twelve other gentlemen were playing cricket, we were surprised to see what we all considered a most novel phenomenon—a *double rainbow*. The sky was cloudy and the weather was thundery. At the time referred to a shower of rain fell; the sun was about 10° above the horizon, shining out very brilliantly and reflecting upon the waters of St. Vincent's Gulf. Great wonder was expressed at the strange appearance, and much curiosity as to the cause.

The appearance was as follows:—There were two distinct and well-defined bows; the feet were united, but the apices were a considerable distance apart.

I am of opinion that the lower bow was caused by the direct light of the sun, while the light reflected from the sea produced the upper one.

THOMAS NOYÉ

Willunga, South Australia, November 30

SCIENCE IN TRAINING COLLEGES

THE Science and Art Department has just issued a circular having an important bearing on the teaching of science is to take in our training colleges, and therefore also in elementary schools.

The Lords of the Committee of Council on Education believe that the time has arrived when a special examination should be instituted at a period of the year better adapted to the training colleges than May, and that the nature of the examination and the payments made on the results should be modified to suit the circumstances of those colleges. They have therefore determined that in future a special examination in science shall be held in training colleges in December, immediately before the ordinary Christmas examination.

The examination will not be open to acting teachers. It will be held in those subjects only for which a special course of instruction is provided in the time-table of the College, and will be conducted by one of her Majesty's inspectors or by an officer of the Science and Art Department. Special committees will no longer be required for the training colleges; such returns as are necessary will be made by the principal. No student in a training college will be allowed to attend the May examinations of the Science and Art Department, except in physical geography in May, 1878.

The examination will be confined to the following nine subjects:—1. Mathematics. 2. Theoretical Mechanics. 3. Applied Mechanics. 4. Acoustics, Light, and Heat. 5. Magnetism and Electricity. 6. Inorganic Chemistry, including Practical Chemistry. 7. Animal Physiology. 8. Elementary Botany. 9. Physiography.

No student will be permitted to take up more than two subjects in any one year, and women will not be permitted to take more than one subject in a year.

The examination, except for mathematics, will be based on the syllabus of the several subjects given in the Science Directory; but the two stages, elementary and advanced, will be treated as a whole—one paper only being set. These examination papers will be framed much as the present May papers are framed, that is to say, with a

certain number of compulsory questions and a certain number of optional questions, some of the latter being more difficult and more highly marked than the rest. Questions will also be set on the method of teaching various branches of the subject.

The successful students will be placed in the first or second class, the standard for a second class being as high as that of a *good* second class in the present advanced stage, and for the first class of a *good* first class in the advanced stage. All students who pass will be registered as qualified to earn payments on results and will receive certificates, but no prizes will be given. A payment of 3*l.* will be made on account of each first class, and 1*l.* 10*s.* on account of each second class obtained by a student in a training college.

In addition to the payments for theoretical chemistry, payments will be made for practical chemistry, of the same amounts and on the same conditions as those detailed in the Science Directory, § XLV. The circular contains an appendix with a syllabus of the subjects for mathematics in training colleges. We should advise all interested in this matter to obtain a copy of the circular.

SUN-SPOTS AND TERRESTRIAL MAGNETISM

I HAVE seen only to-day the number of NATURE (vol. xvii. p. 220) containing a letter from Prof. Piazzi Smyth on the above subject. I have also just now seen for the first time a communication from M. Faye to the French Academy of Sciences on July 30 last, in which there is a reference to the same subject; this I regret much, as M. Faye, through an incomplete acquaintance with my investigations, has drawn conclusions from one of them which are not exact. I shall at present refer only to the subject of Prof. Smyth's letter.

M. Faye considers the difference of the periods found by Dr. Lamont and myself for the diurnal oscillations of the magnetic needle ($10^{\circ}45$ years) and by Dr. Wolf from the sun-spots ($11^{\circ}11$ years), a sufficient proof that these cycles are not synchronous, and therefore that there is no causal connection between the two phenomena. Prof. Smyth asks an explanation relatively to this difference, upon the supposition that the two periods found are the true mean durations of the cycles for the respective phenomena. This supposition, however, is erroneous, and consequently M. Faye's deductions from it fail.

I have shown in a paper cited by M. Faye¹ that if we determine the epoch of the maximum diurnal oscillation of the needle from Cassini's observations made at Paris, and from Gilpin's observations made at London, we find it to have occurred in 1787²⁵. This epoch agrees very nearly with that deduced by Dr. Wolf for the maximum of sun-spots. If we compare this epoch with that of the last maximum which occurred for both phenomena near the end of 1870, we shall obtain a mean duration of $10^{\circ}45$ years, upon the assumption that eight cycles happened between these two epochs. There is no difference between Dr. Wolf and the magneticians excepting upon the question whether there were eight or only seven cycles. Dr. Lamont considers that the data existing between 1787 and 1818 are worthless for a decision upon this point, and by induction from the known cycles has concluded that three cycles must have occurred in the thirty-one years 1787 to 1818. Dr. Wolf believes there were only two. I have given the evidence which makes the existence of three extremely probable. This question has no relation whatever to the synchronism of the two phenomena.

If we could accept Dr. Wolf's view we should find, as I have shown, that the mean duration of a cycle for both phenomena since 1787 would be $11^{\circ}94$ years, while the sun-spot results for eight cycles determined by Dr.

¹ "On the Decennial Period," *Edinb. Trans.*, vol. xxvii.